LETTERS TO THE EDITOR.

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On M. Mercadier's Test of the Relative Validity of the Electrostatic and Electromagnetic Systems of Dimensions.

A SERIES of papers, by M. Mercadier, on the dimensions of physical quantities, has recently been appearing in the *Comptes Rendus*. They are summarised and extended in the *Journal de Physique* (July, 1893, p. 289).

In a note (p. 296) the author states that—"En 1883 nous avons montré, M Vaschy et moi, séparément d'abord, puis en collaboration, que les deux systèmes [of the dimensions of electrical and magnetic quantities] imaginés par Maxwell étaient contradictoires et que l'un d'eux était inadmissible. Depuis, en particulier MM. Hertz en 1885 (*Wied. Ann.* t. xxiv. 1885) et Rücker (*Phil. Mag.* 5^e série, t. xxvii, 1889) sans mentionner notre travail, sont arrivés aux mêmes conclusions."

In general it is, I think, wisest to leave such claims to priority alone, but as M. Mercadier's paper has appeared in two important French journals, I should like to make a few remarks on the history of the dimensional formulæ of electrostatic and electromagnetic quantities, which I do the more readily because I have no claims to priority to establish on my own behalf.

Maxwell's theory leads to the conclusion that between electrical, magnetic, and ordinary dynamical quantities there exist relations which are one less in number than the electrical and magnetic unknowns. Hence the dimensions of all the latter can be expressed in terms of length, mass, time, and of the unknown dimensions of any one of them. Maxwell gave two examples of such expressions in which electrical quantity and strength of magnetic pole are the unknowns selected (vol. ii. first ed. p. 241). It is absurd to suppose that he did not know that similar tables could be drawn up in terms of specific inductive capacity (K) and magnetic permeability (μ).

After discussing the general problem, Maxwell expressed the opinion that the "only systems of any scientific value are the electrostatic and electromagnetic systems" (*loc. cit.* p. 241), and proceeded to explain how they are obtained. Unfortunately, in order to emphasise the fact that the fundamental assumption in the electrostatic system is that the specific inductive capacity of the standard medium (air) is taken as unity, he used a notation in which K is represented as without dimensions instead as of unknown dimensions.

It must also, I think, be admitted that this notation conduced to the use of phrases which might very easily mislead. Thus (*loc. cit.* pp. 368–9) he concludes that a quantity n, which he defines as "the number of electrostatic units in one electromagnetic unit," "is a velocity." Had the symbols K and μ been retained, his argument would have led to the conclusion that $n\,[\mu^{-\frac{1}{2}}\,K^{-\frac{1}{2}}]$ is a velocity. The matter is not of the first importance, but a notation which requires the statement that the ratio of two like things "is a velocity," makes the subject unnecessarily difficult.

Any doubt to which such expressions might have given rise has, however, been completely set at rest since 1882.

A discussion, initiated by Prof. Clausius, then took place in the *Philosophical Magazine* (5th series, vols. xiii. and xiv.), in which Profs. Everett, J. J. Thomson, Lodge, and Larmor took part. It is only necessary for my present purpose to cite the fact that Prof. Lodge explicitly stated that "the number of fundamental relations must be limited by the number of fundamental experiments, viz. three—Coulomb, Coulomb, and Oersted; and the shortest way of writing the independent relations is this:—

$$[\mu e^2] \equiv [Km^2] = [ML]$$

and

$$[\mu K v^2] \equiv 1.$$

The electrostatic convention makes $[K] = 1$; the electromagnetic convention makes $[\mu] = 1$."

This paper was published in September 1882.

In January 1883 a paper by MM. Mercadier and Vaschy appeared in the *Comptes Rendus*. No reference was made by them to the discussion between English and German physicists, but up to a certain point they adopted precisely the same line of argument as that with which we were familiar in England.

Taking the formulæ

$$f = k\frac{qq'}{r^2}$$

and

$$f \propto k'\frac{ii'dsds'}{r^2}$$

they had no difficulty in showing that

$$\left[\frac{k}{k'}\right] = \left[\frac{L^2}{T^2}\right]$$

It was well known in 1883 that Maxwell's theory requires only two constants, K and μ, to define the constitution of the medium, and that

$$[k] = \left[\frac{1}{K}\right],\ [k'] = [\mu]$$

(Maxwell, vol. ii. p. 289, equation 24).

Hence from this point of view the equation

$$\left[\frac{k}{k'}\right] = \left[\frac{L^2}{T^2}\right]$$

is the same as

$$[\mu K v^2] = 1.$$

Up to this point, therefore, there was nothing in the paper of MM. Mercadier and Vaschy which could not be directly deduced by Maxwell's theory from the explicit statements of Lodge.

After this they proceeded to develop the subject further in an argument which may be summarised as follows:—

The constant k is inversely proportional to the specific inductive capacity.

Specific inductive capacity is proportional to the square of the index of refraction.

The index of refraction is inversely proportional to the velocity of light in the medium.

Hence $k = \alpha V^2$, "α etait une constant *numerique*" (the italics are in the original).

Hence, since

$$\left[\frac{k}{k'}\right] = [V^2] \text{ and } [k] = [V^2],$$

k' is a number "et l'emploi du système électromagnetique d'unités électriques se trouverait justifié *théoriquement*."

The fallacy in this is obvious even if the experimental justification of the step $k' \propto$ (refractive index)^2 be admitted.

Because $k \propto V^2$ it does not follow that $[k] = [V^2]$ unless we are sure that all the physical conditions have been included in the equation. Yet Maxwell had given the strongest reason to believe that the magnetic permeability was also involved. He had distinctly pointed out that K would only vary as the square of the refractive index if μ were constant (vol. ii. p. 388). In other words, M. Mercadier, arguing from experiments on materials whose magnetic permeabilities differ but little from that of air, treats $[k] = [V^2]$ or $[K] = [V^{-2}]$ as an independent equation. He thus *assumes* that k' or μ is of no dimensions, and then proves the truth of the electromagnetic system which is avowedly based on that assumption. It would be difficult to find a more complete instance of arguing in a circle.

The point, however, is elaborated by experiment and further argument in another paper (*C. R.* t. xcvi. 1883, p. 250).

The conclusion that k' is a constant is supported by the fact that an induced current is *cæteris paribus* the same, whether the currents are or are not surrounded by non-magnetic materials such as alcohol and benzene.

Again the statement is made—ten years after the publication of Maxwell's book—"D'après les idées universellement admises, les coefficients des formules de magnétisme et d'électromagnétisme seront analogues à k' par consequent ils devraient être comme lui independants des milieux" (p. 252).

The fact is that, according to Maxwell's theory, one of these coefficients is independent of the medium, while the other two vary, the one inversely as the other, when the medium is changed.

Quite apart then from the question as to whether these factors represented pure numbers or concrete quantities, it was at that date almost universally believed that the values of two of them

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depended on the medium. Of course the same view is even more universally held to-day.

All this might, however, have been passed over as an "indiscrétion de jeunesse" if M. Mercadier had not in June last made the extraordinary claim to have proved on such a basis of argument and experiment that the electromagnetic system of units has a theoretical justification which the electrostatic system lacks.

In this recent paper the notation is changed, and k' is used for $1/\mu$. Here again the invariability of this quantity in non-magnetic materials is used as an argument to prove that it does not depend on the nature of the medium.

For the rest M. Mercadier develops certain mixed systems of dimensions, which I need not discuss.

In answer to his complaint that I omitted to notice his memoir in a paper which I wrote on the same subject in 1889, I wish to point out that I did not then enter upon the bibliography of the subject. I regarded myself as dealing with a theory well understood by experts, and as advocating a change in notation chiefly for the benefit of less advanced teachers and students. The considerations advanced were direct deductions from Maxwell's theory. That theory was more generally understood in 1889 than when the discussion in the *Philosophical Magazine* took place in 1882, and since the latter date the practice of retaining K and μ in dimensional formulæ is spreading.

As far, however, as M. Mercadier's papers of 1883 were correct, the ideas they embodied had been explicitly stated in the *Philosophical Magazine* some months before. As far as they went beyond that point, by the attempt to discriminate between the theoretical validity of the electrostatic and electromagnetic systems, the arguments adduced were quite unsound.

ARTHUR W. RÜCKER.

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February 5.

The Cloudy Condensation of Steam.

MR. AITKEN'S letter (p. 340) shows that he has curiously misunderstood me. I never entertained the smallest "objection to" his "not countenancing the nucleus theory to explain" the action of electricity upon the steam jet. On the contrary I was rejoiced to find that so able and distinguished a physicist appeared to hold the same opinion on this point as myself. In labouring to abbreviate I must have become very obscure. Perhaps my meaning may be made clearer by an amplified and annotated paraphrase of the words in question (see *ante* p. 213).

After trying to show that dense condensation takes place only when there is an actual *discharge* of electricity, which, however, need not necessarily electrify the jet, I go on: "The inference clearly is that in some way or other the action is brought about by the *air* in which electrical discharge has taken place, and not directly by the electricity itself. Since so much has been said in the earlier part of the lecture about the influence of dust in promoting condensation the [erroneous] idea has, no doubt, occurred to many of you that in the present case also the air owes its condensing power to the fact that it has become charged with dust. [The great majority of the many scientifically educated people to whom I have at different times shown the experiment at once made this suggestion.] Minute particles are indeed torn off the electrodes by the discharge and [you may think] form nuclei upon which the steam condenses. This [mistaken] hypothesis seems at first sight to be favoured by the experiments of Liveing and Dewar, and by the well-known fact that burning touchpaper induces condensation; it also has the support of Prof. Barus, who appears inclined to think that such condensation is *in all cases* due to the action of small particles of matter. On the other hand, it is noteworthy that Mr. Aitken, who knows more about the condensing property of dust than any man living, gives no countenance to the nucleus theory as explaining the action of electrical discharge upon the steam jet. The possibility of such an explanation must necessarily have presented itself to the mind of one so familiar with the subject, and since he does not make the slightest allusion to it, I imagine that his experiments have led him to the conclusion that it is untenable. This affords me great satisfaction, inasmuch as my own experiments have led me to the same conclusion—not only as regards the action upon the steam jet of electrical discharge, but also of burning matter." [I did not intend to imply, though the words of the abstract apart from the context unfortunately seem to bear that meaning, that Mr. Aitken thought the action of *burning matter* was not due to nuclei, but that I myself thought it was not.] Then follows an account of experiments tending to show that the air does not derive its power of condensing the steam jet from dust but from dissociated atoms.

The above will, I hope, convince Mr. Aitken that, except perhaps as regards one slipshod sentence, which I regret having overlooked when correcting the proof, he has no cause to feel aggrieved. I am confident that my hearers never for a moment understood me to say that he had abandoned one iota of his conclusions regarding the action of dust, but merely that he did not consider the dust-nucleus theory applicable to the case of the electrified steam jet.

I believe that I am well acquainted with all Mr. Aitken's papers on the subject of condensation, but I do not remember the experiment with the polished ball referred to in his letter. Perhaps it is an unpublished one. The experiments which he mentions in his final paragraph, relating to the condensation caused by certain acids, were made upon water-laden air contained in closed vessels, and not upon the steam jet. The conditions in the two cases are very different, so much so that, for example, hydrochloric acid, which in the steam jet is the most active source of dense condensation that I have met with, was found by Mr. Aitken (he will pardon me for reminding him) to form no foggy condensation at all in a receiver of moist filtered air; while ordinary dusty air, which exerts such a powerful action in the closed vessel, fails to produce any sensible effect when introduced into the open steam jet.

SHELFORD BIDWELL.

Southfields, Wandsworth, February 11.

On the Cardinal Points of the Tusayan Villagers.

IN the second volume of the *Journal of American Ethnology and Archæology* I have pointed out, for the first time, that the four cardinal points among the Tusayan villagers are not the same as those of the astronomers, or that their north is approximately north-west. I also gave, in the same article, tables with the amount of the angular variations, showing that the sacred rooms, or kivas, where the mysteries of their ceremonial worship are performed, are oriented, roughly speaking, in accordance with their conception of the positions of north, west, south and east. It was shown that the amount of angular variation was constant, and later, in a description of the ruins of A-na-to-bi, the same orientation was made known.

In an article published in the December number of the *Journal of American Folk Lore*, it was stated by me that the cardinal points among these aborigines are determined by the solstitial risings and settings of the sun.

The publication of Prof. J. Norman Lockyer's work on "The Dawn of Astronomy," in which the orientation of certain of the sun-temples in the Nile valley and elsewhere in the old world is referred to solstitial points in the horizon, gives a new interest to these observations among the aboriginal housebuilders and their descendants in America.

Since the publication (1892) of my observations on the orientation of Tusayan (Moki) kivas and its relationship to solstitial points of sunrise and sunset, I have examined the scanty data which we have regarding the orientation of temples in Central American ruins, and have unearthed significant facts bearing on this question, as well as that of the kinship of the Pueblo people and those who once inhabited the "cities" of Mexico, including Yucatan. Evidences of relationship between the aboriginal housebuilders of Arizona and New Mexico, and those of Nahuatl and Maya stocks have elsewhere been presented. It seems to me that the above observations made in 1891, quite independently of the discoveries of Lockyer on the orientation of temples in the old world, in the light of his discussion, open a field of research in the archæology of the housebuilders of Central America which is sure to lead to interesting discoveries.

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The Scandinavian Ice-sheet.

MANY geologists affirm that the Scandinavian ice-sheet became confluent with that of Scotland, and reached the East

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